

moist winds that blow upon them from the warmer portions of the Atlantic and Pacific Oceans, respectively, and not to any abnormal degree of heat that is conveyed to those coasts through the medium of the ocean currents. To be sure the warm winds and currents appear to accompany each other, but no doubt the currents are more dependent upon the winds for their strength than are the winds upon the currents for their temperature.

METEOROLOGY AT THE BRITISH ASSOCIATION, BELFAST, SEPTEMBER, 1902.

The following extract from the opening address by Prof. Arthur Schuster, Chairman of the Subsection of Astronomy and Cosmical Physics, has so much that is of value to the meteorological student that by special request we reprint it from a recent number of *Nature*:

The question I wish to bring to your notice to-day is an old one: if two events happen simultaneously or one follows the other at a short interval of time, does this give us any reason to suppose that these two events are connected with each other, both being due to the same cause, or one being the cause of the other? Everyone admits that the simple concurrence of events proves nothing, but if the same combination recurs sufficiently often we may reasonably conclude that there is a real connection. The question to be decided in each case is what is "sufficient" and what is "reasonable." Here we must draw a distinction between experiment and observation. We often think it sufficient to repeat an experiment three or four times to establish a certain fact, but with meteorological observations the case is different, and it would, *e. g.*, prove very little if on four successive full moons the rainfall had been exceptionally high or exceptionally low. The cause of the difference lies in the fact that in an experiment we can control to a great extent all the circumstances on which the result depends, and we are generally right in assuming that an experiment which gives a certain result on three successive days will do so always. But even this sometimes depends on the fact that the apparatus is not disturbed, and that the housemaid has not come in to dust the room. Here lies the difference. What is possible in a laboratory, though perhaps difficult, is not possible in the upper regions of the atmosphere, where some unseen hand has not made a clean sweep of some important condition.

When we can not control accessory circumstances we must eliminate them by properly combining the observations and increasing their number. The advantage does not lie altogether on the side of experiment, because the very identity of condition under which the experiment is performed gives rise to systematic errors, which nature eliminates for us in the observational sciences. In the latter also the great variety in the combinations which offer themselves allow us to apply the calculus of probability, so that in any conclusion we draw we can form an idea of the chance that we are wrong. Astronomers are in the habit of giving the value of the "probable error" in the publication of their observations. Meteorologists have not adopted this custom, and yet their science lends itself more readily than any other to the evaluation of the deviations from the mean result, on which the determination of the probable error depends. We look forward to the time when weather forecasts will be accompanied by a statement of the odds that the prediction will be fulfilled.

The calculation of the probability that any relationship we may trace in different phenomena indicates a real connection seems to me to be vital to the true progress of meteorology, and although I have on previous occasions (*Cambridge Phil. Trans.*, Vol. XVIII. p. 107) already drawn attention to this matter I should like once more to lay stress on it.

The particular case I wish to discuss (though the methods are not restricted to this case) is that in which one of the two series of events between which relationship is to be established has a definite period, and it is desired to investigate the evidence of an equal period in the other series.

Connections between the moon and earthquakes, or between sun spots and rainfall if proved to exist, would form examples of such relationships. The question to be decided in these cases would be, is there a lunar period of earthquakes, or an 11-year sun-spot period of rainfall.

Everyone familiar with Fourier's analysis knows that there is a lunar or sun spot, or any other period in any set of events from volcanic eruptions down to the birthrate of mice; what we want to find out is whether the periodicity indicates a real connection or not. Let us put the problem into its simplest form. Take n balls, and by some mechanism allow them to drop so that each falls into one of m compartments. If finally they are equally distributed each compartment would hold n/m balls. If this is not the case we may wish to find out whether the observed inequality is sufficient to indicate any preference for one compartment or how far it is compatible with equality of chance for each. If we were able to repeat the experiment as often as we like we should have no difficulty in deciding between the two cases, because in the long run the

average number received by each compartment would indicate more and more closely the extent of bias which the dropping mechanism might possess. But we are supposed to be confined to a single trial, and draw our conclusions as far as we can from it.

It would be easy to calculate the probability that the number of balls in any one compartment should exceed a given number, but in order to make this investigation applicable to the general problem of periodicities we must proceed in a different manner. If the compartments are numbered, it does not matter in which order, and a curve be drawn in the usual manner representing the connection between the compartments and the number of balls in each, we may, by Fourier's analysis, express the result by means of periodic functions. The amplitude of each period

can be shown on the average to be $\frac{1}{m} \sqrt{\pi n}$. It is often more convenient

to take the square of the amplitude—call it the intensity—as a test, and we may then say that the "expectancy" of the intensity is $4n/m^2$. The probability that the intensity of any period should be k times its average or expectancy is e^{-k} . We may apply this result to test the reality of a number of coincidences in periods which have been suspected. A lunar effect on earthquakes is in itself not improbable, as we may imagine the final catastrophe to be started by some tidal deformation of the earth's crust. The occurrence of more than 7,000 earthquakes in Japan has been carefully tabulated by Mr. Knott according to lunar hours, who found the Fourier coefficient for the lunar day and its first three submultiples to be 10.3, 17.9, 10.9, 39.7; the expectancy on the hypothesis of chance distribution for these coefficients I find to be 19.3, 15.7, 10.6, 5.02. The comparison of their numbers disproves the supposed connection; on the other hand, the investigations of Mr. Davison on solar influence have led to a result much in favour of such influence, the amplitude found being in one series of observations equal to five times, and in the other to fifteen times the expectancy. The probability that so large an amplitude is due to accident in the first case is one in 300 millions, and in the second the probability of chance coincidence would be represented by a fraction, which would contain a number of over seventy figures in the denominator. We may therefore take it to be established that the frequency of earthquakes depends on the time of year, being greater in winter than in summer. With not quite the same amount of certainty, but still with considerable probability, it has also been shown that earthquake shocks show a preference for the hours between 9 a. m. and noon.

A great advantage of the scientific treatment of periodical occurrences lies in the fact that we may determine *a priori* how many events it is necessary to take into account in order to prove an effect of given magnitude. Let us agree, for instance, that we are satisfied with a probability of a million to one as giving us reasonable security against a chance coincidence. Let there be a periodic effect of such a nature that the ratio of the occurrence at the time of maximum to that at the time of minimum shall on the average be as $1+\lambda$ to $1-\lambda$, then the number of observations necessary to establish such an effect is given by the equation $n=200/\lambda^2$. If there are 2 per cent more occurrences at the time of maximum than at the time of minimum $\lambda=0.01$, and n is equal to two million. If the effect is 5 per cent, the number of events required to establish it is 80,000.

To illustrate these results further, I take as a second example a suggested connection between the occurrence of thunderstorms and the relative position of sun and moon. Among the various statistical investigations which have been made on this point, that of Mr. MacDowall lends itself most easily to treatment by the theory of probability. One hundred and eighty-two thunderstorms observed at Greenwich during a period of fourteen years have been plotted by Mr. MacDowall as distributed through the different phases of the moon, and seem to show a striking connection. I have calculated the principal Fourier coefficient from the data supplied, and find that it indicates a lunar periodicity giving for the ratio of the number of thunderstorms near new moon to that near full moon the fraction 8.17 to 4.83.

This apparently indicates a very strong effect, but the inequality is only twice as great as that we should expect if thunderstorms were distributed quite at random over the month, and the probability of a true connection is only about 20 to 1. No decisive conclusions can be founded on this, the number of thunderstorms taken into account being far too small. We might dismiss as equally inconclusive most of the other researches published on the subject were it not for a remarkable agreement among them, that a larger number of storms occur near new moon than near full moon.

I have put together in the following table the results of all investigations that are known to me; following the example of Koeppen, I have placed in parallel columns the number of thunderstorms which have occurred during the fortnight including new moon, and the first quarter and the fortnight including the other two phases.

It will be seen that out of fourteen comparisons, thirteen show higher numbers in the first column, there being also, except in two cases, a general agreement as regards the magnitude of the effect. Two of the stations given in the table, Göttingen and Gotha, are perhaps geographically too near together to be treated as independent stations, and we may therefore say that there are thirteen cases of agreement, against which there is only one published investigation (Schiaparelli) in which the maximum effect is near full moon.

Place of observation and author.	Time of observations.	Percentage of thunderstorms during the fortnight including—	
		New moon and first quarter.	Full moon and last quarter.
Karlsruhe (Eisenlohr).....	1801-31	50.8	49.2
Gotha (Luedicke).....	1867-75	72.5	27.5
Vigevano (Schiaparelli).....	1827-64	46	54
Germany (Küppen).....	1879-83	56	44
Glatz (Richter).....	1877-84	62	38
United States (Hazen).....	1884	56.5	43.5
Prag (Grüss).....	1840-59	51	49
Prag (Grüss).....	1860-79	52.5	47.5
Göttingen (Meyer).....	1857-80	54	46
Kremsmünster (Wagner).....	1862-87	53.8	46.2
Aix la Chapelle (Polis).....	1833-92	54.4	45.6
Sweden (Ekholm).....	1880-95	53.8	46.2
Batavia (Van Der Stok).....	1887-95	51.9	48.1
Greenwich (McDowall).....	1888-91	54	46
Average.....		54.9	45.1

The probability that out of thirteen cases in which there are two alternatives, selected at random, twelve should agree and one disagree is one in twelve hundred. If the details of the investigations summarized in the above table are examined, considerable differences are found, the maximum taking place sometimes before new moon and sometimes a week later. There is, however, evidently sufficient *prima facie* evidence to render an exhaustive investigation desirable. The most remarkable of all coincidences between thunderstorms and the position of the moon remains to be quoted. A. Richter has arranged the thunderstorms observed at Glatz, in Silesia, according to lunar hours, and finds that in each of seven successive years the maximum takes place within the four hours beginning with upper culmination. If this coincidence is a freak of chance, the probability of its recurrence is only one in three hundred thousand. The seven years which were subjected to calculation ended in 1884. What has happened since? Eighteen years have now elapsed, and a further discussion with increased material would have definitely settled the question, but nothing has been done, or, at any rate, published. To me it seems quite unintelligible how a matter of this kind can be left in this unsatisfactory state. Meteorological observations have been allowed to accumulate for years, one might be tempted to say for centuries, yet when a question of extraordinary interest arises we are obliged to remain satisfied with partial discussion of insufficient data.

The cases I have so far discussed were confined to periodical recurrences of single detached and independent events, the condition, under which the mathematical results hold true, being that every event is entirely independent of every other one. But many phenomena, which it is desirable to examine for periodic regularities, are not of this nature. The barometric pressure, for instance, varies from day to day in such a manner that the deviations from the mean on successive days are not independent. If the barometer on any particular day stands half an inch above its average it is much more likely that on the following day it should deviate from the mean by the same amount in the same direction than that it should stand half an inch below its mean value. This renders it necessary to modify the method of reduction, but the theory of probability is still capable of supplying a safe and certain test of the reality of any supposed periodic influence. I can only briefly indicate the mathematical theorem on which the test is founded. The calculation of Fourier's coefficients depends on the calculation of a certain time integral. This time integral will for truly homogeneous periodicities oscillate about a mean value, which increases proportionately to the interval, while for variations showing no preference for any given period, the increase is only proportional to the square root of the time.

Investigations of periodicities are much facilitated by a certain preliminary treatment of the observations suggested by an optical analogy. The curve, which marks the changes of such variables as the barometric pressure, presents characteristics similar to those marking the curve of disturbance along a ray of white light. The exact outline of the luminous disturbance is unknown to us, but we obtain valuable information from its prismatic analysis, which enables us to draw curves connecting the period and intensity of vibration. For luminous solids we thus get a curve of zero intensity for infinitely short or infinitely long radiations, but having a maximum for a period depending on temperature. Gases, which show preference for more or less homogeneous vibrations, will give a serrated outline of the intensity curve.

I believe meteorologists would find it useful to draw similar curves connecting intensity and period for all variations which vary round a mean value such as barometric, thermometric or magnetic variations. These curves will, I believe, in all cases add much to our knowledge; but they are absolutely essential if systematic searches are to be made for homogeneous periods. The absence of any knowledge of the intensity of periodic variation renders it, *e. g.*, impossible to judge of the reality of the lunar effect which Ekholm and Arrhenius believe to have traced in the variations of electric potential on the surface of the earth. The

problem of separating any homogeneous variation, such as might be due to lunar or sun spot effects, is identical with the problem of separating the bright lines of the chromosphere from the continuous overlapping spectrum of the sun. This separation is accomplished by applying spectroscopes of great resolving powers. In the Fourier analysis, resolving power corresponds to the interval of time which is taken into account; hence, to discover periodicities of small amplitude we must extend the time interval of the observations.

I believe that the curve which connects the intensity with the period will play an important rôle in meteorology. It is a curve which ought to have a name, and for want of a better one I have suggested that of *periodograph*. To take once more barometric variations as an example, it is easy to see that just as in the case of white light the periodograph would be zero for very short, and probably also for very long, periods. There must be some period for which intensity of variation is a maximum. Where is that maximum? And does it vary according to locality? The answer to these questions might give us valuable information on the difference of climate. Once the periodograph has been obtained, the question of testing the reality of any special periodicity is an extremely simple one. If h be the height of the periodograph, the probability that, during the time interval chosen, the square of the Fourier coefficient should exceed kh is e^{-k} . If we wish this quantity to be less than a million, k must be about 11; so that in order to be reasonably certain that any periodicity indicates the existence of a truly homogeneous variation, the square of the Fourier coefficient found should not be less than 11 times the corresponding ordinate of a periodograph.

I have calculated in detail the periodograph of the changes of magnetic declination at Greenwich, taking as basis the observations published for the twenty-five years 1871-1895. It was not, perhaps, a very good example to choose, on account of the complications introduced by the secular variation, but my object was to test the very persistent assertions that have been made as to the reality of periodic changes of twenty-six days or thereabouts. The first suggestion of such a period came from Hornstein, of Prague, who ascribed the cause of the period to the time of revolution of the sun round its axis. He only discussed the records for one year's observations, but the evidence he offered was sufficient to impress Clerk Maxwell with its genuineness. Since Hornstein's first attempts, a great many rough and some very elaborate efforts have been made by himself and others to prove a similar period in various meteorological variations. The period found by different computers differed, but there is a good deal of latitude allowed if the rotation of the sun really has an effect on terrestrial phenomena, because the angular velocity of the visible solar surface varies with the latitude. Hornstein himself and some of his followers deduced a period not differing much from twenty-six days, while Prof. Frank Bigelow, using a larger quantity of material, finds 26.68 days, and Ekholm and Arrhenius return to twenty-six days, or, as they put it more accurately, to 25.929 days. The two latter investigators do not, however, adopt the idea that this periodicity is due to the rotation of the sun. None of these periods can stand the test of accurate investigation.¹

¹ It is proper for the Editor to call attention to the fact that Professor Schuster's argumentation in general, but especially his conclusion with regard to Professor Bigelow's period of 26.68 days, had been carefully considered and refuted in a recent publication of the Weather Bureau, which, unfortunately, could not have reached him before he delivered his interesting address at Belfast. On pages 93 and 94 of a Report on Eclipse Meteorology and Allied Problems, published as Weather Bureau Bulletin I, Professor Bigelow has brought out two important points, viz:

1. That the magnetic declinations observed at Greenwich and made the basis of Schuster's calculations do not represent the solar conditions so faithfully, by far, as do the resultants of the vertical and horizontal components which were made the basis of Professor Bigelow's studies. Professor Schuster himself has recognized that there can be no east and west components due to the sun as a magnetic sphere. The variations of the auroras and thunderstorms studied by Ekholm and Arrhenius, and also the variations of declination at Greenwich, as studied by Schuster, are affected by irregularities pertaining to the earth which must be eliminated before we can feel sure that we are studying solar phenomena.

2. Professor Schuster has computed the ratio of the intensity to the expectancy for a number of assumed periods and has made this ratio serve to indicate the probability of the actual existence of the respective periods in the variation of the horizontal declination at Greenwich. These periods and ratios are shown in the following table:

Period, days.	Ratio.	Intensity.
25.809	5.86	0.006168
25.825	4.07	0.004182
25.87	0.95	0.001001
25.929	0.93	0.001027
26.181	1.09	0.001144
26.255	1.04	0.001081
26.68	0.23	0.000242
26.814	5.64	0.005936
27.061	2.80	0.002948

As the result of my calculations, I can definitely state that the magnetic declination at Greenwich shows no period between 25.5 and 27.5 days having an amplitude as great as 6'' of arc. The influence of solar rotation on magnetic variation may therefore be considered to be definitely disproved.

The intensity of the periodograph increases rapidly with the period, and minute variations are therefore more easily detected in short than in longer periods. Six seconds of arc forms about the limit of amplitude, which can be detected in twenty-five years of observations, when the period is about twenty-six days; and from what has been said above, the amplitude which can be detected will be seen to vary inversely with the square root of the time interval. For periods of about fourteen days, an amplitude of 3'' of arc is still distinguishable with the material I have used; and such an amplitude is actually found for a period which has half the synodic month as its time. The chance that this apparent variation is due to an accidental coincidence is one in two thousand; and I can not therefore assert its definite existence beyond all possibility of cavil. But it is surely significant that of all the periods possible between 12.3 and 13.7 days, that gives the highest amplitude which coincides with half the synodic revolution of the moon. That it is at all possible to detect variations of 3'' of arc in the observations which are taken to 6'', with a probability of error of only one in two thousand, is, I think, a proof of the value of the method and the carefulness of the observations. The periodograph has another valuable use. It not only gives us the time necessary to establish true periodicities of given amplitude, but it also gives us an outside limit for the time beyond which an accumulation of material is of no further advantage. That limit is reached when the time is sufficient to discover the smallest amplitude which the instruments, owing to their imperfections, allow us to detect.

I am only concerned to-day with a purely statistical inquiry, and not with the explanation of any suggested relationship. To prevent misunderstandings, however, I may state that I consider the possibility of a direct magnetic or electric action of the moon excluded; as regards the latter, the diurnal variations of electric potential would be so much affected by a lunar electrification sufficiently strong to influence the outbreak of thunderstorms that it could not have escaped discovery. We must not, however, be dogmatic in asserting the impossibility of indirect action. The unexpected discovery of radio-activity has opened out an entirely new field, and we can not dismiss without renewed careful inquiry the evidence of lunar action which I have given. Its reality can be decided by observation only. No—not by observation only—but by observation supplemented by intelligent discussion; and this brings me to my concluding appeal, which I wish to urge upon you with all the legitimate weight of strong conviction and all the illegitimate influence of presidential infallibility.

The subjects with which our subsection is concerned deal with facts which are revealed to us by observation more frequently than by experiment. There is in consequence a very real danger that the importance of observation misleads us into mistaking the means for the end, as if observation alone could add anything to our knowledge. Observation is like the food supplied to the brain, and knowledge only comes through the digestion of the food. An observation made for its own sake and not for some definite scientific object is a useless observation. Science is not a museum for the storage of disconnected facts and the amusement of the collecting enthusiast. I dislike the name "observatory" for the astronomical workshop, for the same reason that I should dislike my body to be called a food receptacle. Your observing dome would be useless without your computing room and your study. What you want is an astronomical laboratory, a meteorological or magnetic laboratory; attaching to the word "laboratory" its true meaning, which is a workshop in which eyes and hands and brains unite in producing a combined result.

The problems which confront the astronomer being more definite than those of meteorology, astronomy has grown under the stimulus of a healthy tradition. Hence, it is generally recognized, at any rate in the principal observatories, that the advance of knowledge is the chief function of the observer. Nevertheless, the president of the astronomical department of section A last year (Prof. H. H. Turner) has found it neces-

sary, in his admirable address, to warn against the danger there is that the astronomer should allow himself to be swallowed up in a routine work and mere drudgery. The descent is easy: You begin by being a scientific man, you become an observer, then a machine, and finally—if all goes well—you design a new eyepiece.

If such a danger exists in astronomy, what shall we say about meteorology? That science is bred on routine, and drudgery is often its highest ambition. The heavens may fall in, but the wet bulb must be read. Observations are essential, but though you may never be able to observe enough, I think you can observe too much. I do not forget the advances which meteorology has made in recent years, but if you look at these advances, I think you will find that most of them do not depend on the accumulation of a vast quantity of material. The progress in some cases has come through theory, as in the applications of thermodynamics, or through special experiments, as by kite and balloon observations, and when it has come through the ordinary channels of observation, only a comparatively short period of time has been utilized. It would not be a great exaggeration to say that meteorology has advanced in spite of the observations and not because of them.

What can we do to mend matters? If we wish to prepare the way for the gradual substitution of a better system, we should have some one responsible for the continuation of the present one. For this purpose it should be recognized that the head of the meteorological office is something more than a secretary to a board of directors; also that he is appointed to conduct meteorological research and not to sign weather forecasts. The endowment of meteorology should mean a good deal more than the endowment of the telegraph office which transmits the observations.

WEATHER BUREAU MEN AS INSTRUCTORS IN METEOROLOGY.

According to the published course of studies at Cornell University for the year 1902-3, meteorology is included under geology and physical geography, and two courses are offered to the students by Prof. R. S. Tarr and Section Director R. G. Allen. The course in elementary meteorology occupies two hours a week in the first half year, and the course following this, entitled "Study of Weather Bureau Methods," occupies two hours a week for the whole year. Mr. Allen writes that in the academic year 1900-1901 10 students took the course in elementary meteorology; in the year 1901-2 there were 27; in the current year 1902-3 about one hundred applied but only 48 could be accommodated. In addition to these, two students who have finished the course in elementary meteorology are now taking the course in elementary methods and will take the Weather Bureau examination on October 21.

In the College of Agriculture meteorology appears to find no place, but in the College of Forestry it is one of the courses prescribed for the freshmen year. Doubtless, however, there are some students in the College of Agriculture who take meteorology as an elective study, and one of the seniors in the agricultural course also contemplates taking the Weather Bureau examination.

Mr. W. H. Alexander, Observer, Weather Bureau, reports that on September 18 he delivered an address on the Weather Bureau and the Farmer before the conference of agricultural teachers at Rio Piedras, Porto Rico, W. I. This address was enthusiastically received and was followed by a number of talks by the teachers on cooperation with the Weather Bureau.

W. A. Shaw, Observer, Northfield, Vt., reports that he addressed farmers' institutes in Maine at Perry, August 13, and East Machias, August 14.

JOHN T. PROBERT.

It will be observed that the intensities are less than they should be on the theory of chance, and it will also be noticed that there is about the same probability or ratio for the periods 25.809 and 26.814. Of course both of these can not be simultaneously correct, so that Schuster's method of discussion has failed to find any period that conforms near enough to Greenwich observations to justify speaking of it as a law of nature. But this failure does not affect the question as to the existence of a true period in the solar phenomena; it simply shows that the Greenwich observations of declination are probably not the proper data upon which to base any such research. As Professor Bigelow's work is based upon the observation of both horizontal and vertical components for many years at stations scattered over the whole globe, his data, therefore, represent actual solar conditions as nearly as these can be determined from terrestrial observations and must give us results of the highest probability. Notwithstanding the expense, it is to be hoped that these original data and laborious computations may be published in full as an important contribution to the study of the relation between the earth and the sun.—Ed.

Another of our oldest voluntary observers has passed away. Mr. John T. Probert, of Paterson, N. J., was born in West Drayton, Middlesex, England, in 1829 of Welsh parentage. When he was 18 years of age his family settled in Paterson, N. J. He learned the trade of a cobbler and developed a large business in the manufacture of shoes. He was an omnivorous reader and his biographer in the Paterson Daily Press states that even at an early age he had acquired a comprehensive library. He was a close student of nature, an ardent lover of geology, and an enthusiastic lover of meteorology.